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CYCLIC AND NON-CYCLIC ASPECTS OF EROSION[1]

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GEOLOGY has always centered around an effort to decipher records. Until fifty years ago these records were almost exclusively those of the constructional processes, conspicuously those made by sedimentation. Erosion has always been a great destroyer of records. Down to the last half century it was scarcely thought of as offering any compensation by recording a history of its own.

Within that time its records have been analyzed with increasing insight. They are made rapidly and in great abundance but always at the surface. Hence they are much like characters written on wax tablets, always destroyed to make way for newer letters. Only occasionally is a tablet discarded and buried and the writing thus preserved. Such a record is an unconformity. Erosional history mentions relatively few large facts of early geologic date, but of recent events the account is very full, even more so than the parallel account written in the language of deposition. The study and interpretation of the records left by erosion constitute the larger part of the science of geomorphology.

These records begin to have value when erosion is seen as a series of events and not as a continuous process without beginning or end or variation. It is true that erosional events had long been used to chapter, paragraph and punctuate the sedimentary record, but the post-sedimentary record was a kind of unsystematic epilogue, without plot, added merely to tell what became of the characters. From a human standpoint it contained some things of news value, but the whole was rather less orderly than a morning paper. Items of significance were not yet organized into a science.

With Powell's concept of the "base level of erosion"

[1] Address by the retiring president of the Geological Society of America at its New York meeting, December 26, 1935.

came recognition of the fact that even without submergence and renewed deposition, the work of erosion may come to definite stops. This was a great advance. It divided the whole of post-sedimentary erosion into definite undertakings, each task (if completed) beginning with an uplift and ending at base-level. Thus was the foundation laid for Davis's cycle.

In order to be serviceable the cycle concept, like any other, must have definite meaning. It is not enough that a land mass is reduced to base-level, then lifted up and reduced again. If the word "cycle" means no more than that, the word "repetition" would have done as well. The cycle finds its character in a regular round of events and changes, always in the same order. Progress is marked by changing form, and the stage attained is known by appropriate topographic features as truly as though they were hands on a clock. Many topographic features need no other explanation than merely to point out their position in the cycle.

If, on the other hand, a surface be thought of as lowered by erosion, while remaining constantly parallel to its first position, there is no propriety in speaking of a cycle. The consideration of this case may be deferred. It is first necessary to examine the results of a series of cycles.

In a series of complete cycles it is obvious that there would be no record of any but the last. The "complete cycle," however, remains an intellectual ideal, unless the area be restricted by cutting off margins where the work was incomplete. Probably no river ever ran to the sea carrying nothing but water. Anything else would indicate that somewhere in its basin the work was still in progress. The common experience is to find in a single area the partially destroyed, or newly begun, forms of several cycles, none of them carried to completion, unless it be the first. The work of the next cycle stopped somewhat short of the stage reached in the first, the third fell short of the second, and so on. Speaking only of those whose records remain, it follows that the cycles were of decreasing completeness and (presumably) in most cases, of decreasing duration. The prevalence of this observation is sometimes noted as curious, as though implying that the earth's crust is becoming progressively less stable. Reflection shows, however, that the record could never have been otherwise, since only such cycles are recorded as were followed by others less complete. The limitations of the sedimentary record, inherent in the method of its making, have often been remarked, but there is less consciousness of the principle that erosion is limited in this respect, that it can record only a series of diminishing cycles. Another Schooley cycle would wipe out the whole story.

It is unnecessary at this point to explain at length the exceptional effects of local base-levels. It is obvious that the principle here stated is true only when the same area is affected by all the cycles, *i.e.*, in a succession of regional uplifts. There are many local cycles and local peneplains related to local base-levels, each independent of the others. These are not considered in the principle just stated. But the assumption that a series of peneplains reflects changes of base-level throughout the area concerned carries with it the assumption of an accelerated succession of diastrophic events. A very long series would seem to involve serious issues on which the geophysicist may well ask to be heard. If a dozen or a score of base-levels be evidenced, the probability becomes very great that most of them were local. With respect to the Appalachians this same conclusion has been reached by other lines of reasoning.

In considering the probability or improbability of numerous cycles, or a multiplicity of peneplains, another principle must be taken into account. Valleys near the sea reflect very promptly the effects of small ups and downs which remain forever unrecognized in the great interior. Davis saw this nearly 50 years ago and wrote:

> The cutting and filling resulting from comparatively brief and trivial elevations and depressions [near the sea] make a record so complete and so complicated that its details encumber the problem and place its solution out of reach for the present.[2]

With due attention to the two principles here mentioned, the vast proliferation of cycles and peneplains between the Hudson and the Potomac may be in part removed from controversy. Yet not entirely, for geologists are still human beings, and the subjective factor is always present in their conclusions. Even when the objective data are agreed upon, the pattern seen in the mind's eye may be one thing to one man and something quite different to another. A simple and familiar illustration of this is the projected profile, on which one man may see a flight of stairs (say six peneplains) where another sees only a general slope, interrupted by fortuitous ups and downs. If half a dozen hills have approximately the same height, one sees a peneplain, the other mere coincidence. Such subjective differences are perfectly normal, and neither type of mind can afford to scorn the other. Time will bring about a slowly emerging consensus of opinion which will probably be right.

The interpretation of erosion in terms of cycles is based largely on the recognition and identification of peneplains. The concept of the peneplain is by no means so well defined as the offhand and frequent use of the term might seem to indicate. In exact discus-

[2] W. M. Davis, *Bull. Geol. Soc. Amer.*, 2: 577, 1891.

sion the user of that word must still state what he means. Whatever else the term stands for, it certainly designates the ultimate or penultimate stage of the normal cycle, but the amount of residual relief allowed is determined subjectively by every man for himself.

A peneplain is "almost a plain" but there is no single and convenient word that designates "almost a peneplain." Such a word is sorely needed. For lack of it the term "peneplain" has come to be overworked, being used with more and more allowance for imperfections. This is because poor peneplains are more numerous, and perhaps "almost a poor peneplain" is still more abundant. Yet even this last may be very arresting and very significant when it constitutes the surface of a highly complex structure of great magnitude. If successful inquiry could be made into what lies in the back of the minds of those who speak of peneplains, it would probably be found that the only impression always there is that the likeness in altitude among divides is sufficient to suggest a common control and that no other control than a possible base-level is obvious. This is a very vague commitment. In mountain countries like the Sierra Nevada, such a surface, or the imagined generalization of such a surface, becomes a plane of reference above which mountains are seen to rise and into which valleys are cut. It may safely be asserted that any degradational surface so thought of is bound to be called a peneplain, even though its own relief is many hundreds of feet. That is, the term will be so used until some other short and euphonious term takes its place. This was not the intention of the maker of the term and is not here defended as right, but only pointed out as fact, even a regrettable fact.

At all events, no peneplain was ever flat. Even on the Schooley there were local swells, ridges and hills, and probably the major valleys were several hundred feet lower than the major divides. Much criticism and not a little cynicism toward peneplains has been aroused by expecting too much. It is not in their nature to be flat. The outcropping edges of strong formations, like the Tuscarora in Pennsylvania, the Berea in Kentucky, the Burlington in Missouri, the Niagara in Wisconsin, the Winslow in Arkansas and the Chase group in the Flint Hills of Kansas, may continue almost indefinitely to make low swells or subdued cuestas on peneplains. When the plains are lifted up and the escarpments again sharpened, the surfaces on opposite sides of the escarpment are mistakenly assigned to different cycles. Thus the Highland Rim and Lexington peneplains are made two instead of one. The same is true of the Salem and Springfield uplands in the Ozarks. And the Driftless Area of the Upper Mississippi is allotted an undue number of cycles and peneplains.

Both in the original intent and in current usage the term peneplain connotes a mode of origin quite as much as of form. There are other ways of producing plains perfect or approximate. A generation ago it was still necessary to discuss the criteria which distinguish the plain of marine planation from that of subaerial degradation. Whatever the difficulties of practical application, there is in this case no confusion in thinking, hence no need for discussion.

But if confusion of thought is no longer to be feared in the case of marine planation, it certainly is in another, that is, in the case of stream planation. The lateral swing of a meandering stream makes, not a peneplain, but a flat. Such a surface is not made by wearing down but by sawing off. The two processes are wholly and essentially different. So are the resulting forms, despite their superficial resemblance.

It is true that these two types of topography are often associated. It is normally to be expected that when a region is worn down to low gradients, its streams will meander broadly. Lateral planation and alluvial plains will be extensive, but these, taken by themselves, are not peneplain in any exact or technical sense, while the worn-down areas between them are true peneplain in their own right. Speaking geographically and with reference to large areas, the flood-plains may be included, just as peat bogs are included in a ground moraine, but peat bogs are not moraine, and flood-plains are not peneplains. To assume the complete planation of an area by the meandering of its streams and merging of its flood-plains is simply to dispense with the idea of peneplanation altogether and to substitute another process. Yet even in recent literature, and in the work of able geologists who are morphologically minded, there is seen, now and again, an implication of identity of the peneplain and the alluviated planation surface. An expression like the following is typical: "(Streams) began at once to lower their channels in the old peneplain, and, when they had reached the new base-level, to form a new peneplain by lateral corrasion."[3] When it is remembered that planation surfaces are relatively numerous, the consequences of treating each one as a peneplain will readily be seen.

In this connection attention should be given to the association of peneplains with gravel. Gravel is to be expected in the alluvium associated with a peneplain as in any other alluvium on a suitable grade, but within that area that is typical peneplain and not something else, nothing is scarcer or less expectable than gravel. When a surface, suspected on other grounds of being a peneplain, is found to contain patches or strips of gravel at about the general level, the evidence of peneplanation is strengthened. But

[3] C. W. Hayes, *Nat. Geog. Monograph* No. 10, p. 330, 1906.

if the entire area is gravel-covered the assumption is that it is a planation surface, made by a stream which may or may not have been flowing on a peneplain. Generally the gravel-free surface is much the larger part of the area. This is about equivalent to saying that search for gravel must be limited to those parts which are only peneplain by courtesy, *i.e.*, by reason of the company they keep. Gravel may corroborate the suspicion of a peneplain, only when its elevation is approximately that of a more extensive surface. Alluvium, whose nature bespeaks a very low gradient, affords presumptive evidence, not proof, of a peneplain.

This matter is important, for it bears directly on the multiplication of peneplains by confusion with river terraces. The latter have their own value in giving evidence of a changing base-level and thus of interrupted cycles of erosion. They have their uses in the study of diastrophic history, but the time intervals which they record are relatively short, and generally they have nothing to do with completed cycles.

The confusion of flood-plains with peneplains is in many cases a matter of terminology. Rarely would the one be mistaken for the other. It is otherwise with structural plains. So impressive is the cycle, so vastly useful and illuminating in the study of history and topography, so widespread is the evidence that land masses have been reduced to peneplains, that almost any flat horizon is under immediate suspicion of owing its flatness to a former base-level. One can scarcely be surprised if such control is sometimes invoked where the flat horizon is in reality due to some other cause or condition.

Foremost among these other conditions is the presence, immediately beneath the surface, of a strong stratum which may have arrested erosion at that level. Where such a relation is suspected geologists at once line up, those who are cycle-minded opposed to those who are structure-minded. These traits may be congenital or they may have been instilled, but whether one or the other they are permanent factors in determining the judgments of their possessors. Often they fix the judgment in advance and forestall investigation. A man simply belongs to one party or the other, just as he is a Republican or a Democrat, or (if you are a reader of Alphonse Daudet's "Tartarin of the Alps") he belongs to the party that takes prunes for dessert or the party that takes rice. Debates will go on indefinitely between the cycle-minded and the structure minded, just as between individualists and socialists, or between any other groups whose line-up is fixed by sympathies. In the end each group will say that the other needs to study the question in the field, a kind of final thumbing of the nose with which geologists end a hopeless argument.

It seems highly probable that the influence of resistant strata has often been underrated (and the number of base-levels correspondingly overestimated) especially in dissected plateaus. It is difficult to read the literature of the Appalachian plateaus and avoid this impression. The reason for limiting this statement to dissected plateaus and excluding extensive uncut stratum plains will appear later. From the most recent treatment of the Appalachian plateaus in Pennsylvania, it might be concluded that altitudes owe more to two Carboniferous sandstones than to any succession of cycles. Whether or not this statement is too strong, it calls attention to the fact that the attempt to divide up a great area and assign each and every segment to the work of some particular cycle runs into grave difficulties and must be so compromised as virtually to be abandoned. Much of the area, even where the skyline is flat, may be only remotely related to any peneplain.

The simplicity and beauty of the conception of allotting all parts of an area to their respective cycles is alluring. So much so that we are prone to think in terms of diagrams, in which each higher level gives way visibly to a lower and younger surface, a newer peneplain which is constantly enlarging at the expense of the older and constantly losing by the spread of still newer and lower surfaces. The conception embodied in such diagrams is so simple, so illuminating, so useful, in many cases so true, and it burst so suddenly upon the science, hitherto without it, explaining so many things, and introducing order where chance had reigned, that it can not be wondered at if its application was, for a time, made too broad.

The recognition here given to structure as a determining factor in horizontal surfaces is believed to be a just concession. No service can be rendered to the cycle theory by overstraining it. But explanation in terms of structure can also be overstrained. To minds of a certain twist there is something hypnotizing in the presence of a structural surface which happens to parallel the topographic surface. The two seem to be necessarily related, like the unexplained sounds in a strange room and the number 13 over the door. The fact that structural surfaces must have *some* position is apt to be overlooked. More of them are horizontal or nearly so than in any other one attitude. The same is true of topographic surfaces. By the mere law of hazard the surface must often parallel the structure. Yet the discovery of such agreement is often treated as sufficient evidence of cause and effect. Sometimes this is true, as every student of plateaus knows. Often it is not.

There is such a thing as a plain of stripping, quite independent of base-level, but the limitations of stripping at high levels are very severe and are often

ignored. To do a clean job of stripping a horizontal bed is just as difficult as to make a perfect peneplain. The process is the same, and its last stages are just as slow in one case as in the other if equal areas be assumed. The time required increases enormously with the extent of the area. Meantime the margins, stripped early in the process, must hold their own against dissection and this dissection is a very speedy process if the relative altitude is considerable. The result is that while rock terraces and mesas of limited extent are common, extensive plains, stripped at considerable altitudes, are in all cases subject to dispute. The stripping is not denied; only the altitude at which it is done. The question at issue pertains to the ability of any hard stratum to maintain a local base-level high above the sea, during the long time required for peneplanation.

A familiar example is the Edwards Plateau in Texas, 1,000 to 3,000 feet above the sea and 400 to 1,200 feet above its surroundings. The underlying limestone formations, collectively known as the Edwards, are relatively resistant in the climate of central Texas. No doubt this is the explanation of the plateau's present height above the lowlands on weaker rock. But it does not follow that the stripping was done at that altitude. The margins of this formation are being raked and shredded in a way to show the precarious position of the entire mass at its present altitude. In view of the present havoc (obviously of recent beginning) one wonders how the margins of the plateau retained their flatness during the long time required to strip the interior. If unstable now, why were they not unstable then? And why is the process of destruction still in its early stage? The evident answer is that the stripping was not accomplished at the present altitude.

This region is described in the classical paper of Hill and Vaughan.[4] In descriptive terms the limestone surface is said to be stripped, but it is also pointed out that the several beds are beveled, the surface being here on one bed and there on another. This is one of the tests, if not *the* test, of a peneplain. To say that the Edwards Plateau has been stripped is a mere statement of fact; but to leave the inference that the strength of the limestone explains the flat surface without regard to the control of base-level during erosion is wholly unwarranted.

An even more striking illustration is seen in the "Great Sage Plain," 6,000 to 7,000 feet high in southeastern Utah and southwestern Colorado. Its agreement with the surface of the Dakota sandstone is noteworthy. It is mentioned as one of the best American examples of stripped plain, and generally with the implication that the strong Dakota sandstone was, in this case, an adequate substitute for base-level. However, it is only necessary to look at the sharp canyons, already branching into the interior, to be convinced that the Great Sage Plain can not last long under present conditions. Its prompt dissection will be a brief episode in comparison with the long, tiresome process of washing away the last stains of Mancos shale from the still flat surface. Obviously this slow stripping process must be allowed a long handicap and must have run most of its course before dissection was allowed to start.

It should be a safe principle that a peneplain can not originate under conditions which make it essentially unstable. So slow a process as peneplaning must not be asked to run a race against so swift a process as dendritic dissection of a high plateau. Applying this principle to the Great Sage Plain, it is safe to conclude that, at the time of its development, the altitude was much less than at present, certainly not high enough above the local base-level to make dissection possible.

Within limits of altitude and of time, a strong stratum may become a substitute for base-level, but these limits are, without doubt, much narrower than are implied by many casual descriptions of stripped plains. The strong stratum may actually raise the local base-level a little for a long time, or may raise it much for a short time, but not much for a long time. It follows from this that, in terms of diastrophic events, the interpretation of an extensively stripped horizontal stratum, now at considerable altitude, differs very little from that of a peneplain. Each was necessarily developed at a lower altitude.

What is said here of the stripped or denuded stratum applies equally to the exhumed or resurrected peneplain. The mere fact of exhumation implies that the rocks below are strong. The discovery that the present surface agrees essentially with one of pre-Cambrian time is sometimes hailed as proof that the present nearly flat surface is not a true peneplain. This may be true if the patches concerned are small. Barring this limitation the more recent plain may be treated much like a peneplain in its own right; that is, it may be so regarded in the interpretation of diastrophic history.

In view of what was said before about structural control, expressing the belief that it has received something less than its due in the Appalachians, this may seem to some like blowing hot and cold with the same breath. Structural control is sometimes found to be adequate and sometimes not. If difficulty is found in stripping ten thousand square miles but not in stripping ten square miles, a single strong stratum may be made to dominate the horizon over a large area by

[4] U. S. Geological Survey, Eighteenth Annual Report, Part 2, 1808.

the simple expedient of dissecting it first and stripping afterward, or allowing the two processes to go on simultaneously. Such an explanation is not necessary for most of the Appalachian plateaus, at least not until some one disputes the fact that they were largely or approximately base-leveled in the Schooley cycle. But it helps in minor ways, as in the avoidance of an older and higher base-level on the Harlan sandstone in the Cumberland Mountains.

The problem of accounting for a multiplicity of surfaces (topographic or mathematical, actual or conceptual) at different levels is a main, if not *the* main, center of controversy in most discussions of cyclic history. The above remarks on the influence of structure bear on this subject. But there is another factor, recognized though not yet prominent in discussions, which may produce surfaces at an indefinite number of levels. This is the slow wasting of a surface without change of characteristic form. This is no new discovery. It was tacitly assumed before the cycle was born. It would scarcely be going too far to affirm that the coming of the cycle, with its more exact concept, its orderly sequences, its concreteness and its exemplification in known forms, has done much to withdraw attention from a vague, universal, unobservable process whose results show poorly in diagrams. Yet such a process is going on, and it is lowering most of the earth's surface, sometimes in association with valleys, sometimes without.

Such erosion, without systematic change of form, is essentially non-cyclic,[5] for the cycle produces and reproduces a series of events and forms. One stage is not like another. There is a beginning, a climax and an end. Cycles have parts, and the parts make wholes, and the wholes may be counted like apples. Non-cyclic erosion can only be measured like cider. There is neither part nor whole but only much or little. The exact altitude of a surface affected by such erosion does not record an event, but only a continuing process which may be fast or slow. Obviously surfaces under such conditions may have almost any altitude. A mere count of altitudes would mean little in terms of events.

It may need emphasizing that the cycle itself is not a physical process but a philosophical conception. It contemplates erosion in one of its aspects, that of changing form. But erosion does not always and everywhere present this aspect. This generalization is not apt to be denied; in any case the exact physical process does not concern us here. It is sufficient to say that the potentialities of erosion without producing valleys will bear far more emphasis than they have received.

So constant is our association of valleys with erosion that it is difficult to think of the straight, horizontal Appalachian crests as being lowered scores or even hundreds of feet and yet looking the same after as before. The fact has long been acknowledged, though not until recently has it been given much significance. Hayes was first to suggest a possible 300 feet (in the south). Recently Ashley[6] has made a minimum estimate of 100 feet in a million years.

Considering first the case of narrow ridges like those of the Ridge and Valley province, it is to be observed that this process of surficial wasting does not destroy the horizontality of a crest but only lowers it. If the amount of such wasting were everywhere the same, the record of cyclic erosion would not be defaced and the count of cycles and peneplains would not be confused. Confusion begins when one ridge has been lowered 30 feet and another 300 feet. Both crests received their flatness at the same time, *i.e.*, when both summits were parts of the same peneplain. Neither one has at any time lost its flatness. Both are lowering now as fast as ever and neither is at base-level. Yet a casual view, and perhaps the present vogue, would assign them to different cycles with the tacit implication that neither summit has been lowered since uplift and that the summit plane of each cuts the mass now just where it did when the peneplain was made.

Theory would indicate that the rate of erosion without valleys should vary with the hardness of the rock and the width of the outcrop, the latter being determined by thickness of stratum and dip. Even a casual examination of the Appalachian ridges is sufficient to indicate that such correlations of altitude with structure exist. Thick strata make higher ridges than thin ones, and the ends of pitching folds where the outcrops are broad are almost invariably high. Much ingenuity has been expended in depicting a series of base-levels so that each mountain crest may fall in one of the assumed planes. When an equal amount of exact study shall have been given to correlating each height with the character of rock and the breadth of outcrop, the time will have come to decide how many base-levels must be assumed. Perhaps three would be enough, or two; the last and extremest suggestion is one. More than three may be needed.

Application of the principle of constant, universal and unequal degradation of crests is based in part on actual observation and correlation. The inherent probabilities in the case are almost equally deserving

[5] The substance of what is said here about the non-cyclic aspect of erosion was presented by the writer before the Chicago Geological Society on March 8, 1933. The same principles were applied to the interpretation of the Allegheny Plateau in a paper read before the Geological Society at its meeting in Chicago in December, 1933, and published in abstract on page 78 of the Proceedings of that year.

[6] Geo. H. Ashley, *Geol. Soc. of Amer. Bull.*, Vol. 46, 1935.

of attention. But such a consideration may also save us from going too far and limiting the number of cycles too severely. Judging from the sedimentary record it is inherently probable that cycles of all grades and degrees of completeness should appear in the post-sedimentary record. Indeed, short aborted cycles, resulting in local peneplains on soft rocks, are much more probable than such a remarkable occurrence as the Schooley cycle. It is not strange that such an exceptional event as that left the most convincing records. Yet logic would scarcely permit us, on that account, to accept the reality of the extraordinary and not to acknowledge the ordinary. A Harrisburg cycle, having some such importance as custom ascribes to it, would be quite in line with the expectable.

A vivid portrayal of the effects of unequal wasting is seen in the slant of some ridge crests (not the axes of plunging anticlines) which can not be assigned to any one peneplain, one end perhaps agreeing in height with an older base-level, and the other with a younger. What we know with absolute certainty about the Ridge and Valley province is that there are ridges or parts of ridges at all possible heights from minimum to maximum; there may well be more than one height that seems to be specially favored. This is presumptive evidence (though not proof) of more than one base-level.

Turning now to plateau surfaces, unexpected reenforcement of this principle of erosion without change of form has recently come from our newborn concern with soil erosion. It has long been known that gullying of fields is one of the major wastes of our civilization. Now it appears that even this widespread and spectacular disaster is a secondary matter compared with the skimming of top-soil with no observable change in topographic form. Bennett affirms[7] that in addition to the 50 million acres of gullied land in the United States, and another 50 million acres "about as bad," there are 125 million acres (nearly 200,000 square miles) all or most of whose top-soil has been carried away in the short period of cultivation. The Soil Conservation Service is undertaking to determine by direct observation the rate of such soil wastage without gullies. As a check on their methods and results it may be noted that the ascertained rate of wastage of moderately steep ungullied grass-covered slopes on loess in Missouri is not very different from the computed rate for the entire Mississippi basin, based on the observations of Humphreys and Abbott on the annual load of the Mississippi River. The rate of erosion is of course excessive on ploughed fields. But the actual rate of such lowering is a secondary matter. No application of this principle to geomorphology assumes more than a minute fraction of the higher rates observed in soil studies. Even at Ashley's rate of 100 feet in a million years the Allegheny Plateau would have lost a bare eighth of an inch since its settlement.

It is unnecessary to say that this erosion without valleys is in part dependent on steepness of slope. After making due allowance for the work of wind and solution, a perfectly flat upland should be narrowed rather than lowered. This means that the altitude of a plateau in its pre-mature stages is not significantly reduced. From maturity on, all crests are melting down concurrently with the wasting of slopes. With a stable base-level, lowering of crests and flattening of slopes would proceed together after the familiar pattern of the plateau cycle. With continuous or intermittent uplift of suitable amount, the steepness of the slopes and the depth of the valleys may continue unchanged while divides are pared down hundreds of feet. There is no theoretical limit to the amount of uplift and erosion that a maturely dissected plateau may undergo without change of characteristic form. Meanwhile, in another portion of the same original plateau, but consisting of harder rock, erosion may have lagged behind uplift. The result will be two adjacent plateaus, differing in elevation and, according to customary interpretation, representing two cycles.

In its application, this principle is related to that of stripped plains, but the two are not identical. The essence of this latter view is that all planes are being reduced at all times, but some faster than others. Indeed, computations or estimates of rate made by geologists have been based almost wholly on the wasting of the more resistant rocks in mountains.

The exact rate at which this general wasting proceeds is not a matter of primary concern. The suggested rate of 100 feet in a million years is somewhat slower than the general wasting of the Mississippi basin. The important consideration is that, if the rate of lowering is any considerable fraction of this amount, no elevated surface older than Pleistocene is properly interpreted without taking this factor into account. If all surfaces were lowered at the same rate the remains of former peneplains would still stand in their true relative positions. But no one will assert that all surfaces waste equally. The result is that all correlations based on altitude are liable to error in proportion (among other things) to the antiquity of the surfaces concerned. Before correlation, all readings must be corrected by an amount dependent (to say the least) on time, slope and resistance. This correction is not a minute Einsteinian matter. For features dating from Miocene time it may well run high into hundreds of feet. To compute such correc-

[7] H. H. Bennett, SCIENCE, 81: 322, 1935.

tions for peneplains older than late Tertiary is an arduous task. To correlate without them should be classed by law as among the dangerous occupations.

It is no reflection on the cycle to point out that its records are subject to complication with those of another process. Rather, it should be apparent that, when the complicating factor has been properly evaluated and allowed for, the record of cycles will be less confused and more trustworthy. It seems appropriate to speak of this other factor as essentially non-cyclic. It matters little whether the terminology here used be liked or accepted. It matters much that the facts be recognized. For want of such recognition the cycle is burdened with so many complexities and inconsistencies as to impair its usefulness or even at times to expose it to unfriendly criticism.

THE NATIONAL MAPPING PLAN OF THE NATIONAL RESOURCES BOARD

By Dr. WILLIAM BOWIE
CHIEF OF THE DIVISION OF GEODESY, U. S. COAST AND GEODETIC SURVEY

FOR many years scientific men, including engineers, have realized the importance to a country of having a complete knowledge of its terrain and all physical data relating to the earth's surface. Without basic facts good plans can not be made, and they surely can not be carried out with any degree of effectiveness.

It is recognized that the topographic map shows graphically essential data regarding the area covered. It would take years of traveling or of reading in order to obtain a knowledge of an extensive region, while with a topographic map much more comprehensive and accurate knowledge can be gotten in a few hours.

Not only is the topographic map of value in planning the industrial and commercial activities of our people, but it is essential in many lines of scientific research. The configuration of an area has a bearing on plant and animal life. Without a knowledge of the terrain geological and geophysical investigations can not be carried on with efficiency and accuracy.

In spite of the need for maps, very little has been done. Only about 47 per cent. of the area of the United States, about three million square miles, has been covered by topographic maps and more than half of those maps are so out of date or so sketchy in character that they do not meet present-day needs. We thus see that only about 25 per cent. of the area of the country is adequately mapped; in fact, many of the otherwise satisfactory maps must be revised to show changed cultural features.

The National Resources Board, an agency set up by President Roosevelt to advise on the conservation and utilization of our resources, requested the Federal Board of Surveys and Maps to prepare a plan for completing the mapping of our country. This was done late in 1934. This plan was endorsed by the National Resources Board and was forwarded to the President in one of its reports. The opening paragraph of this report is significant:

> Most of the land planning and land use agencies of the Federal Government, as well as many other Federal and State organizations whose activities are concerned with land, have asked the Board of Surveys and Maps to prepare a program for the completion at an early date of the mapping of the United States. The Board has made an exhaustive investigation and finds much evidence that the actual loss of money due to lack of adequate maps is greater than the estimated cost of completion of the standard map of the United States. Moreover, most of the land use agencies have testified that the absence of adequate map data makes it almost impossible to carry out any plan of readjustment in land use until the areas affected are adequately mapped.

This report in turn has been commented on favorably and given endorsement by the Science Advisory Board of the National Academy of Sciences.

The national mapping plan was considered of such interest by the American Society of Civil Engineers that it was printed in full in the February, 1935, issue of *Civil Engineering,* a journal of that society.

Many engineers, geologists, biologists and others have expressed great interest in the national mapping plan and have expressed the hope that it might be put into effect immediately and carried on vigorously. It calls for the completion of the topographic mapping of the country within ten years at an estimated cost of $117,531,000 or less than $12,000,000 per year. Congress had already authorized the mapping of the country as a federal project in the so-called Temple Act, which became a law on February 27, 1925. That act authorized the appropriation of funds with which to complete the map of the country within twenty years. Ten years have passed and little topographic mapping has been done during that time. Forty-three per cent. of the country had been mapped before 1921, while to-day only 47 per cent. has been mapped. This is at the rate of 0.3 of one per cent. per year. One can see that the mapping will not be completed within a hundred years at this rate of progress, and besides,

a map should be revised at frequent intervals, say of five to ten years, in order that it may show new conditions as to cultural features.

The Temple Act authorized the acceptance by federal bureaus charged with mapping the country of funds from states or subdivisions thereof, which might wish to have the mapping of their areas expedited. It was not mandatory that the states should make contributions towards the mapping.

Under the present national mapping plan, provision is made whereby funds may be received for expediting the mapping in any particular area or in making larger scale or more detailed maps than what are ordinarily called the standard maps.

Topographic maps must be based upon geodetic or control surveys which furnish the latitudes, longitudes and elevations of great numbers of monumented points on the earth's surface, together with distances and azimuths between adjacent points. With emergency funds the Coast and Geodetic Survey has during the past three years added much to the control survey nets of the country. There are now in the United States about 261,000 miles of lines of levels, and 67,000 miles of arcs of triangulation. The plan that has been followed is to have the lines of levels and arcs of triangulation spaced at intervals of approximately 25 miles. So far as the leveling is concerned, the 25-mile spacing has been completed to the east of the 102° meridian. To the west of that meridian some additional leveling must be done to secure the 25-mile spacing. There remains approximately 47,000 miles of arcs of triangulation to finish the 25-mile spacing of the net.

Much more control surveying will be needed to furnish the basis for topographic mapping. Within the 25-mile meshes of the nets will be needed horizontal and vertical control survey stations placed at intervals of from 5 to 7 miles in order to supply the topographic engineer with a sufficient number of control stations.

Airplane photography will no doubt be used to a great extent in the mapping of the country. The data shown on the photographs taken from the air can be fitted into the control stations that may be plotted on the map, and thus topographic features, such as roads, railroads, shore lines, rivers, streams, forests, settlements and even individual houses, can be placed on the map in their true geographic positions. The so-called planimetric map made from airplane pictures can be taken to the field and the topographic engineers can draw on them the contours representing elevations and slopes. There are also now in use a number of instruments with which contours can be plotted from airplane pictures, using optical methods. This is a new and as yet very limited development in topographic mapping.

Undoubtedly the mapping plan will be put into effect, but just how soon no one knows. When we consider the cost of completing the mapping of the country in terms of human energy or man-years, we find that the cost to our people is almost insignificant. Measured in dollars we may think it is high. However, ten million dollars per year for each of ten years would be the equivalent of the cost of only 2,000 miles of modern highways or 200 miles per year. We are spending hundreds of millions of dollars per year on highways and I feel confident that if we had good maps from which to select the routes for the highways, great economies would be effected in the highway work. The best routes could be selected and the maintenance charges for the roads based upon the maps would be less than for those highways constructed in unmapped areas.

Elevation and slope bear a definite relationship to animal and plant life. Maps would more than justify their cost by their use in the plant and animal industries of the country alone. With the maps there could be far better control of those organisms that influence favorably or unfavorably plant and animal life.

I suppose that the real reason that the mapping of the country has not been carried on more vigorously in the past is that there has been no concerted action on the part of those whom the maps would benefit. Individual action does not carry as much weight as does united effort based on unified opinion. Whether there will be a concentration of effort on the part of map users to have the plan put into effect, no one knows, but it is reasonably certain that we are becoming map-minded. It is unfortunate that we have so few maps in a map-minded age.

SCIENTIFIC EVENTS

PLANS FOR THE DEVELOPMENT OF THE ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA

A DEVELOPMENT program which will make the Academy of Natural Sciences an active part of Philadelphia's educational system was announced by its president, Effingham B. Morris, at the formal opening on January 16 of the new East African water-hole habitat group in the Free Museum of the academy. Dr. James Bryant Conant, president of Harvard University, and Dr. William Berryman Scott, emeritus professor of geology at Princeton University, who

spoke at the gathering, urged the carrying out of this program which embodies these three main objectives.

The meeting was attended by prominent educators, naturalists and museum officials of Philadelphia and other cities, as well as by representative Philadelphians in social and civic circles. Mr. Morris, who gave the opening address, gave an outline of the history of the academy. He said that when the trustees began to study what should be done to meet increasing public demands, many avenues of approach suggested themselves. They were finally narrowed down to three main objectives for immediate consideration, as follows:

> The first objective is the further development of our museum. We want a museum where *ideas* are on display rather than *things*. We are, therefore, starting a study that will result in a definite plan based upon the best advice and authority that we can get.
>
> Second, we are going to render a more active service to the school children of Philadelphia. Last year some 30,000 came to the academy, some from such relatively distant points as Atlantic City and Reading. It is our intention to build a plan whereby the academy will provide them with a more concrete service. There are many things that can be done—guides, special courses, loan collections, trips, lectures, and natural science clubs are only a few of them.
>
> Third, and last of these first steps, is the reestablishment of a department of paleontology. In this field we have the opportunity of building a department that does not duplicate the work of near-by institutions, and of filling a great gap. The academy has the collections and a great heritage in the work of such men as Leidy and Cope. Given a scientist who can put their collections in order, and make them available for study, we can render a real service. And I want to emphasize the fact that this is merely the entering wedge—the instrument that will show the practicability of closer cooperation with *all* the departments of the academy.

This program will be carried out under direction of an executive committee headed by Arthur E. Newbold, Jr., treasurer of the academy and a member of its board of trustees. The following trustees will serve as chairmen of the three committees: Museum Development, Frank B. Foster; Educational Cooperation, J. Stogdell Stokes; Department of Paleontology, Edgar B. Howard.

ANNUAL REPORT OF THE DIRECTOR OF THE NEW YORK BOTANICAL GARDEN

AT the annual meeting on January 13 of the trustees of the New York Botanical Garden Henry W. de Forest was reelected president; Henry de Forest Baldwin was reelected vice-president; John L. Merrill vice-president and treasurer, and Dr. Marshall A. Howe director and secretary.

Elective managers chosen for a 3-year term include Arthur M. Anderson, of Bedford, N. Y.; Clarence Lewis, of Sterlington; E. D. Merrill, of Harvard University (who until October 1 was director of the garden); Lewis Rutherford Morris, of New York City; Dr. Marshall A. Howe, Henry W. de Forest and Henry de la Montagne, Jr. Mrs. Samuel Seabury, Edward C. Delafield, Dr. Tracy E. Hazen, Mrs. Henry J. Fisher, Mrs. Frederick A. Godley, Mrs. Geo. McM. Godley, Mrs. William F. Hencken, Mrs. Henry F. Schwarz and Mrs. Townsend Scudder, were elected to membership in the corporation.

Dr. Howe, who before his election as director had been assistant director of the garden for eleven years, presented his first report. He emphasized horticultural displays of the garden and its increased services to the public.

Reviewing the scientific achievements of the staff he referred to the breeding of useful and decorative varieties of plants, research into the background of this work, and the identifying of many thousands of pressed plants. He reported the addition of nearly 54,000 specimens to the herbarium, raising the total number of specimens to 1,800,000. He acknowledged the botanical books that have appeared in the last year under the signature of staff members—notably Dr. H. A. Gleason's "Plants of the Vicinity of New York" and Dr. John K. Small's "Ferns of the Vicinity of New York." He called attention to the work of the 100 to 200 WPA employees, who have given substantial help during the year in clerical, scientific and specialized work indoors, and in gardening and construction and repair work both inside and out. He spoke at length on the numerous floral exhibits of 1935, through which the garden had been able to acquaint the public with suitable plants in great variety for out-of-door and indoor growth.

Cooperating with other institutions, the garden during 1935 gave away more than 48,000 duplicate herbarium specimens, and received nearly as many different ones in exchange. In addition, more than 17,000 sheets of pressed plant material were sent out on loan for students all over the world.

As a special service to members, choice shrubs, waterlilies, begonias and iris were distributed during the year. Six thousand packets of seed were mailed to 134 other botanical gardens and institutions, besides individuals, and 4,730 packets were received in return.

Dr. Howe also called attention to courses given at the garden, which are attended by members, professional gardeners, teachers and others.

THE NORTH AMERICAN WILDLIFE CONFERENCE COMMITTEE

IN calling the North American Wildlife Conference for February 3–7, President Roosevelt said:

My purpose is to bring together individuals, organizations and agencies interested in the restoration and conservation of wildlife resources. My hope is that through this conference new cooperation between public and private interests, and between Canada, Mexico and this country, will be developed; that from it will come constructive proposals for concrete action; that through these proposals existing state and federal governmental agencies and conservation groups can work cooperatively for the common good.

F. A. Silcox, chief of the United States Forest Service, was appointed chairman of the conference by the President, and a committee was designated to organize and develop a program.

Dr. Silcox says: All organizations and individuals interested in the restoration and conservation of wildlife are invited to attend and participate. Three major objectives constitute the fundamental purposes of the conference, as follows:

(a) The organization of a permanent general federation of all agencies, societies, individuals and clubs interested in the restoration and conservation of wildlife resources with the avowed purpose of securing adequate recognition of the needs of wildlife resources;
(b) The development of a North American program for the advancement of wildlife restoration and conservation;
(c) The presentation of such facts, discoveries and information pertinent to wildlife as may contribute to the solution of our mutual problems.

A tentative proposal of methods for the permanent organization of a general federation of all wildlife conservation organizations and individuals will be presented by the committee on organization which will be subject to open discussion, modification and adoption by the conference. This federation if it is to be successful must confine itself to major objectives of national and state efficiency and support for recognized common aims. Such aims may be presumed to be:

(a) The comprehensive restoration and conservation of wildlife resources;
(b) Adequate financial support from public funds;
(c) Recognition of conservationists in the selection of conservation executives;
(d) Demand wildlife representation on state and federal resource and planning agencies and that in authorization of projects financed by public funds which affect land or waters important to wildlife adequate consideration be given to the biological consequences;
(e) Effective registration of mass opposition to activities which have proved to be inimical to wildlife interests and which are without adequate justification for public good.
(f) By a consideration of the international aspects of wildlife, develop a program which would unite the interests of the nations involved.

The Mayflower Hotel, Connecticut Avenue, Washington, D. C., has been selected as general headquarters. All meetings will be held there except the daily mass meetings, which will be in the Connecting Wing Auditorium on Constitution Avenue. Requests for further information, and all communications, should be addressed to Mr. Silcox.

THE GOLD MEDAL AND FELLOWSHIPS OF THE AMERICAN INSTITUTE, NEW YORK

THE Gold Medal of the American Institute will be presented to Dr. John C. Merriam, secretary of the Carnegie Institution of Washington, at the annual dinner meeting, which will be held on February 6 at the Hotel Pierre at 7:30 P. M.

The medal is awarded to Dr. Merriam "for his discoveries in paleontology, his effective promotion of research and his recognition of the place of science in man's affairs."

Fellowships of the institute will be presented to Dr. Harrison E. Howe "for service to science and industry by the interchange of research results among investigators throughout the world and by creating a better public understanding of the functions of research and its place in the advance of civilization." To Howard W. Blakeslee "for his contribution to man's fuller understanding of the world in which he lives by accurate presentation of the news of science in the language of the layman and for his service to the development of science through fostering the public's interest in its advances."

The medal and fellowship presentations were made as follows by Alfred Knight, president of the institute:

Presentation of Dr. Harrison E. Howe by Dean Carl William Ackerman, School of Journalism, Columbia University. Address by Dr. Howe, editor of *Industrial and Engineering Chemistry*, "Science As News."

Presentation of Howard W. Blakeslee by Dean Ackerman. Address by Mr. Blakeslee, science editor, Associated Press, "Influence on the Public of Science News in the Press."

Presentation of Dr. John C. Merriam by Dr. William K. Gregory, professor of Vertebrate Paleontology, Columbia University; curator, Department of Comparative Anatomy, and Department of Ichthyology, American Museum of Natural History. Address by Dr. Merriam, president, Carnegie Institution of Washington, "Science and Human Values."

RECENT DEATHS AND MEMORIALS

DR. HENRY TABER, since 1921 professor emeritus of mathematics at Clark University, previously since 1888 docent and professor, died on January 6. He was seventy-five years old.

ARTHUR MACDONALD, at one time specialist in the U. S. Bureau of Education, the author of many publications on anthropology and criminology, died on January 17 at the age of seventy-nine years.

FOSTER HENDRICKSON BENJAMIN, associate entomologist in the Bureau of Entomology and Plant Quarantine of the U. S. Department of Agriculture, died on January 21. He was forty years old.

THE Albany Medical College and Hospital announces that it has named the new laboratory building "The Theobald Smith Memorial Laboratory" in recognition of his outstanding scientific achievements. Dr. Smith was a native of Albany, where he was born on July 31, 1859. He attended the city schools and was graduated from the Albany High School from which he went to Cornell University. In 1883 he received the degree of doctor of medicine from the Albany Medical College.

THE trustees of the American Museum of Natural History have adopted a resolution directing that a bust of the late Henry Fairfield Osborn, for twenty-five years president of the museum, be placed in the memorial hall of that institution in commemoration of his work. The trustees also decided that Professor Osborn's rooms in the southeast tower, where he worked, should be set aside as a place for paleontological research.

THE *Journal* of the American Medical Association states that a room containing historical material has been set aside at the University of California Medical School in honor of the late Dr. Leroy Crummer, Los Angeles, who inaugurated systematic instruction in medical history in the school. The exhibit includes Dr. Crummer's library and donations by physicians. Dr. Crummer, who died in 1934, was clinical professor of medical history and bibliography at the medical school and professor of the history of medicine, University of Southern California School of Medicine.

Nature reports that a marble plaque has recently been affixed to the birthplace at Villeneuve-le-Guyard of Professor Chauveau, who was born there in 1827 and died in Paris in 1917. At the time of his death, he was a member of the Academy of Medicine, honorary professor of the Veterinary School at Lyons, professor at the Paris Museum and inspector of the French Veterinary Schools.

SCIENTIFIC NOTES AND NEWS

THE F. Paul Anderson Gold Medal for distinguished scientific achievements in the field of heating, ventilating and air conditioning has been awarded to Dr. Arthur Cutts Willard, president of the University of Illinois, by the American Society of Heating and Ventilating Engineers. The presentation was made at the forty-second annual meeting of the society held in Chicago on January 29. The faculty and officers of administration of the University of Illinois will honor Dr. Willard at a function on the evening of February 3. The occasion is for the purpose of "expressing our appreciation of the action of the board of trustees in selecting as president a distinguished member of our group. We can also express to President Willard our admiration for his work as a member of the faculty and for his leadership since he became president in July, 1934."

FRANK W. CALDWELL, engineering manager of the Hamilton Standard propeller division of the United Aircraft Manufacturing Corporation of East Hartford, Conn., has received the Sylvanus Albert Reed Award of the Institute of the Aeronautical Sciences. This award, consisting of a certificate and $250, was endowed with a bequest of $10,000 by the late Dr. S. A. Reed and is presented annually for "a notable contribution to the aeronautical sciences resulting from experimental or theoretical investigations, the beneficial influence of which on the development of practical aeronautics is apparent." Mr. Caldwell received the presentation at the annual dinner of the institute, held at Columbia University on January 30, for "increasing the effectiveness of aircraft through development and improvement of controllable and constant-speed propellers."

DR. E. V. MCCOLLUM, professor of biochemistry at the Johns Hopkins University School of Hygiene and Public Health, has been presented with the Callahan Memorial Award by the Ohio State Dental Society. This award is a gold medal given each year to a person "who has made a contribution to dental science which is of very exceptional value."

OFFICERS of the American Physical Society were elected at the St. Louis meeting as follows: *President,* Dr. F. K. Richtmyer, professor of physics and dean of the Graduate School of Cornell University; *Vice-president,* Dr. H. M. Randall, professor of physics and director of the physical laboratory of the University of Michigan; *Members of the Council,* Dr. J. W. Beams, professor of physics at the University of Virginia, and E. C. Crittenden, physicist of the Bureau of Standards. *Members of the board of editors of The Physical Review,* Dr. H. A. Bethe, Dr. L. A. DuBridge, assistant professor of physics, Washington University (St. Louis) and Dr. M. A. Tuve, physicist at the Carnegie Institution of Washington.

DR. ROBERT W. HEGNER, professor of protozoology at the Johns Hopkins University, has been elected president of the American Society of Parasitologists.

AT the first meeting of the newly organized Limnological Society, Professor Chancey Juday, of the University of Wisconsin, was elected president and Professor R. E. Coker, of the University of North Carolina, was elected vice-president.

DANIEL W. MEAD, from 1904 until his retirement as emeritus professor in 1932 professor of hydraulic and sanitary engineering at the University of Wisconsin, assumed office as president of the American Society of Civil Engineers at the eighty-third annual meeting, which was held in New York from January 15 to 18. He succeeded Arthur S. Tuttle, state engineer and acting state director for the Federal Public Works Administration of the state of New York.

AT the St. Louis meeting, the biological society of Beta Beta Beta elected the following officers: *President,* Professor C. E. McClung; *Regional Vice-presidents,* Dr. Wyman R. Green, Dr. W. K. Butts, Dr. C. L. Furrow, Dr. G. E. Potter and Dr. A. E. Noble. The secretary of the society is Dr. F. G. Brooks, of Oklahoma City University.

AT the thirty-eighth annual meeting of the Washington Academy of Sciences on January 16, the following officers were elected for 1936: *President,* O. E. Meinzer, Geological Survey; *Corresponding Secretary,* Nathan R. Smith, Bureau of Plant Industry; *Recording Secretary,* Charles Thom, Bureau of Plant Industry, and *Treasurer,* H. G. Avers, Department of Commerce. *Vice-presidents* representing the affiliated societies were also elected as follows: Philosophical, Francis B. Silsbee; Biological, Charles E. Chambliss; Chemical Society, James H. Hibben; Entomological, A. H. Clark; National Geographic, Frederick V. Coville; Geological, W. T. Schaller; Medical, H. C. Macatee; Columbia Historical Society, Allen C. Clark; Botanical, Charles Drechsler; Archeological, Aleš Hrdlička; American Foresters, S. B. Detwiler; Washington Society of Engineers, Paul C. Whitney; American Institute of Electrical Engineers, H. G. Dorsey; Mechanical Engineers, H. N. Eaton; Helminthological, Emmett W. Price; Bacteriologists, H. W. Schoening; Military Engineers, C. H. Birdseye; Radio Engineers, Louis Cohen.

THE title of emeritus has been conferred by the Ohio State University on Dr. Chas. B. Morrey, formerly professor of bacteriology at the university.

DR. A. S. WHEELER, since 1900 a member of the department of chemistry at the University of North Carolina, is, on account of illness, on indefinite leave of absence. Until last autumn Dr. Wheeler filled the post of acting head of the department of chemistry, being named to that position following the death of Dean James M. Bell. Dr. Edward Mack, Jr., of the Ohio State University, was appointed head of the department last fall and assumed his new work in September.

DR. MILTON J. ROSENAU, who retired last February as professor of preventive medicine and hygiene in the Harvard Medical School and professor of epidemiology in the School of Public Health, has been appointed director of a new Division of Public Health, established as a part of the Medical School of the University of North Carolina. The purpose of organizing the department is to train students to be health officers. The department was founded by the university with the cooperation of the North Carolina State Board of Health.

DR. JOHN A. WILSON has been appointed acting director of the Oriental Institute of the University of Chicago, to succeed the late Professor James H. Breasted.

DR. NORMAN L. MUNN, assistant professor of psychology at the University of Pittsburgh, has accepted a professorship of psychology at the George Peabody College for Teachers, Nashville, Tenn. He succeeds Dr. Joseph Peterson, who died on September 20, 1935. Dr. Munn will assume his new duties early in February.

LOUIS JORDAN, senior metallurgist, chief of the section of thermal metallurgy and assistant chief of the division of metallurgy of the National Bureau of Standards, has been appointed assistant secretary of the American Institute of Mining and Metallurgical Engineers. He will serve as secretary for the Iron and Steel and for the Institute of Metals divisions. He will take up his work at the headquarters in New York City beginning on February 1.

SHERMAN M. WOODWARD, head of the department of mechanics and hydraulics at the State University of Iowa, has resigned his professorship to accept a position as chief water control planning engineer with the Tennessee Valley Authority at Knoxville, Tenn.

GEORGE W. TRAYER, senior engineer on the staff of the Forest Products Laboratory at Madison, Wis., has been appointed chief of the division of forest products in the U. S. Forest Service, Washington, D. C. This position, newly established, involves liaison activities designed to integrate wood utilization research with other federal forestry activities throughout the United States. Mr. Trayer's headquarters will be in Washington.

NEW appointments at the American Museum of Natural History include: Dr. Frederick H. Pough, assistant curator in the department of mineralogy;

G. Miles Conrad, assistant curator in the department of comparative and human anatomy, and Charles E. Federer, Jr., staff assistant in astronomy and the Hayden planetarium. John Saunders has been promoted from staff assistant to assistant curator in the department of education and Dr. C. H. Curran, from assistant curator to associate curator in the department of entomology.

DR. W. CARSON RYAN, JR., who has been on leave from his position as director of education of the U. S. Indian Service, engaged in a field study of mental hygiene and education for the Commonwealth Fund of New York, has definitely severed his connection with the Indian Service.

PROFESSOR A. O. RHOAD, of the Instituto de Pesquisas Agronomicas, Pernambuco, Brazil, who has leave of absence, has returned to the United States. He plans to spend several months at the Institute of Animal Nutrition at the Pennsylvania State College.

DR. E. V. MCCOLLUM, professor of biochemistry at the Johns Hopkins School of Hygiene and Public Health, sailed on January 29 for Geneva. There he will attend as the representative of the United States the International Commission on Nutrition sponsored by the Health Section of the League of Nations. In November he attended a similar session held at the London School of Tropical Medicine. Delegates from the Scandinavian countries, England, France and Russia attended this session. The Commission on Nutrition is a permanent organization recently set up by the League of Nations. Its members include economists and statesmen as well as those skilled in the science of nutrition.

DR. W. C. GEORGE, of the School of Medicine of the University of North Carolina, is spending a few months at the Bass Biological Laboratory continuing his work on the comparative study of Ascidian blood.

DR. ALEXIS CARREL, member of the Rockefeller Institute for Medical Research, has been appointed Hitchcock professor to lecture during the spring semester at the University of California.

THE fifth lecture of the Harvey Society will be given at the New York Academy of Medicine on February 20 by Dr. John F. Fulton, Sterling professor of physiology, Yale University School of Medicine, on "The Interrelation of Cerebrum and Cerebellum in the Regulation of Somatic and Automatic Functions."

DR. GEORGE W. MCCOY, of the National Institute of Health, gave on January 16 the address of the retiring president before the Washington Academy of Sciences. He spoke on "Comings and Goings of Epidemics."

DURING January, Professor Bailey Willis, of Stanford University, delivered a series of six lectures on "The Crust of the Earth" at Columbia University.

PROFESSOR ARTHUR H. COMPTON delivered a series of lectures at Ohio University on January 7, 8 and 9. The titles of the lectures were: "Determination of Atomic Constants," "Freedom and Causality in Modern Physics," "X-rays and Atomic Structure," "The Quest of the Cosmic Ray," "Science and Modern Civilization."

Nature states that at the invitation of the Royal Society, the North East Coast Institution of Engineers and Shipbuilders will arrange the first of the annual lectures to be held in connection with the memorial to the late Sir Charles Parsons. The institution has nominated Sir Frank Smith, secretary of the Department of Scientific and Industrial Research and since 1929 secretary of the Royal Society, to deliver the lecture and the council of the Royal Society has approved the nomination. The lecture will be held at Newcastle-upon-Tyne, probably in the latter part of 1936.

THE annual meeting of the British Royal Microscopical Society was held in London on January 15, when Professor W. A. F. Balfour-Browne will deliver his presidential address on "The Evolution of Social Life among the Insects."

THE celebration of the one hundred and twelfth anniversary of the foundation of Birkbeck College (University of London) was held in the College Theater on Wednesday, January 22. The foundation oration was delivered by Sir Frederick Gowland Hopkins, past president of the Royal Society.

THE two hundred and fourth regular meeting of the American Physical Society will be held in New York on Friday and Saturday, February 21 and 22, as a joint meeting with the Optical Society of America. All sessions will be held at Columbia University in the Pupin Physics Laboratories, on 120th Street east of Broadway.

PLANS for the seventieth anniversary of the founding of engineering courses at Lafayette College are being made for the ceremonies to be held on March 20 at Easton. A general committee headed by Professor Moreland King, of the department of electrical engineering, has been making plans for exhibits, speakers and dinners. Dr. William S. Hall, professor emeritus of mathematics, from 1884 to 1915 instructor and professor of mining engineering, will deliver the historical address entitled "Seventy Years of Engineering at Lafayette." Besides Professor Hall's address, there will be a convocation speaker for the morning session to be held in the Colton Memorial Chapel. The

services will be preceded by an academic procession of trustees, guests and faculty. At the afternoon session, following a luncheon given to the visiting engineers, a conference on engineering problems will be held. The ceremonies will close with a dinner and evening session.

GRANTS recently made by the Rockefeller Foundation include a grant of not more than £1,200 a year for five years from January 1 for research in cellular physiology at the Molteno Institute of Parasitology of the University of Cambridge, under the direction of Dr. David Keilin, Magdalene College, Quick professor of biology; $2,000 to the Medical School of the University of Oregon, for research on the physiology of the brain by Dr. William F. Allen and Dr. Olof Larsell, professors of anatomy; $1,000 to Union College to provide apparatus, animals and chemicals for the use of Dr. Samuel L. Leonard, assistant professor of biology, for his researches in endocrinology.

THE University of Hamburg is henceforth to be known as the Hansa University.

PROFESSOR HANS MEYER, the editor of *Strahlentherapie*, Park Allee 73, Bremen, on the occasion of the fortieth anniversary of the discovery of Roentgen rays, has issued an appeal to radiologists of all nations for portraits and short biographies of medical men, physicists, nurses and others who have lost their lives as the result of x-ray work.

THE trustees of Lake Forest College will award a cash prize of $15,000 for the best book or manuscript, heretofore unpublished, on the connection, relation and mutual bearing of the humanities, the social sciences, the physical sciences, the biological sciences, or any branch of knowledge, with and upon the Christian religion. The award will be made under the Bross Foundation, established in 1857, after decision by a committee of judges, on or after January 1, 1940.

THE Dutch Genetic Association has offered a prize of 250 gulden for an essay on the inheritance of differences in resistance to disease in men and animals. The essay must contain a review of the literature, especially as regards diseases of the blood, with personal observations and conclusions. The essay should be sent to the secretary of the society, Dr. A. L. Hagedorn, Soesterberg, Holland, before a date to be announced later.

DISCUSSION

SOME ZOOGEOGRAPHICAL PROBLEMS OF THE NORTHERN PACIFIC

THE northernmost parts of the Pacific Ocean have been little studied by marine zoologists, and many zoogeographic problems there have escaped the attention of scientists. This is not only due to a lack of work in the area, but also depends on the tendency among the older zoologists to describe most specimens from such "remote places" as "new species" after superficial examination. The literature is thus encumbered with a considerable number of Pacific species which are either so scantily described that they can not be reidentified, or can not be separated from Atlantic species by existing figures and descriptions.

Every zoogeographical study must be based on an exact and detailed knowledge of species and even of the smallest varieties, as in ecology, a useless study without such information. This also applies to causal zoogeography. In many cases small and insignificant characters such as those used in classification (taxonomy) by geneticists in their heredity studies should be used.[1]

The general zoogeographical studies of the Pacific coastal waters of North and Central America have, contrary to earlier expectations, revealed remarkable differences there from the fauna of Eastern Asiatic waters. On the other hand, there is an obvious relationship between the Atlantic littoral fauna of Central America and the (much richer) one of the Indo-Malayan Archipelago. Sven Ekman has recently described in detail[2] the recent evidence of these relationships. Ekman reviews the fate of the Thetys Ocean and the ecological factors determining the spread of littoral animals. The data suggest that the great abyss of the Pacific Ocean between the Paumotu Islands and the western coast of America was an effective barrier to the littoral fauna at the time of the Thetys Ocean and has remained so ever since, causing the gap in the tropical littoral fauna on the two sides of the Pacific. The comparative poverty on the American side of the Atlantic as compared with the Indo-Malayan region does not prove the Atlantic fauna to be younger. The relationship between the two faunas are features in common derived from the Thetys fauna, and the scarcity on the Atlantic side is due to a period of low temperature in Miocene and Pliocene times. The problems thus brought up by Ekman make it desirable to study the littoral fauna of the northern Pacific. While the Asiatic waters especially in the vicinity of Japan and from there to Bering Straits—an area

[1] Comp. V. Brehm, "Ueber die tiergeographische Valenz der Speziesmerkmale," Zoogeographica, Bd. 1, Jena, 1932.

[2] "Indo-Westpazifik und Atlanto-Ostpazifik, eine tiergeographische Studie," Zoogeographica, Bd. 2, Jena, 1934.

recently investigated by the Russians—are well known, the waters on the American side have scarcely been studied, although this region is one of especial interest.

In the northern parts of the Pacific, there are some species which are also known from the North Atlantic. As many of these "boreal" species can not pass through warmer waters, they must have crossed the Polar Sea by a passage now barred to many of them by the low temperatures of the Arctic Sea. There may be other barriers, as pointed out by E. Gurjanova.[3] She also discusses the various routes of dispersal of Arctic species from their centers of origin in the Polar Sea, the northern Atlantic or Pacific Oceans. This is also discussed by Leo S. Berg.[4] From studies of the Molluscan fauna and particularly from the work of W. H. Dall, he concludes that the boreal and subarctic fauna in the upper Tertiary had favorable conditions for development in the northern Pacific. In the Miocene and Pliocene this fauna apparently prevailed throughout the ocean which at that time stretched from the Kamtchatkan region to the northern Atlantic. In post-glacial times, an exchange between the northern Pacific and Atlantic boreal territories through the Arctic Sea was possible during the higher temperatures of the Ancylus and Littorina periods.

My investigation of the Russian collections of Octocorals from the northern Pacific ("Oktokorallen des nördlichsten Pazifischen Ozeans und ihre Beziehungen zur atlantischen Fauna") corroborates Berg's conclusions. A study of the oceanographic features of the Arctic Sea makes it probable that species like the Octocorallians *Paragorgia arborea, Primnoa resedaeformis* and *Pavonaria finmarchica* can not have originated in the Atlantic, but must have invaded the northern Atlantic through the Arctic Sea when the temperature was somewhat higher than at present, and when the entrance to the Arctic Sea from the Pacific was deeper than it is now, *i.e.,* probably at times when great parts of Alaska were submerged. It must be assumed that the ocean currents, especially those of the Arctic, were essentially the same as they now are.

At present, the Atlantic and Pacific stocks of many boreal (northern, but not arctic) species are effectively isolated from one another. This is due to the low temperatures of the Arctic Ocean (especially from the New Siberian Islands to the northern Alaskan region) which, combined with the current flowing thence northward, entirely block the eastern passage against Atlantic intruders north of the New Siberian Islands. Furthermore, the boreal species living in the deeper strata can not penetrate through the shallow waters of Bering Strait.

It is a fundamental assumption of glacial geologists that a species always has the same environmental needs, regardless of age and habitats. This axiom may be doubted without definite proof. (1) Are the demands of a morphologically constant species unchanged during the ages and under various environmental conditions so that it can not stand changes in temperature, salinity, etc.? (2) If a stenovalent species can adapt itself to changed conditions, is this adaptation necessarily combined with changes in its morphologic features?

There are examples among terrestrial animals that changes of conditions brought about by experiments may in certain species change their ecology.[5] But among marine animals, there is no evidence of this in the literature. One might mention results of aquarium experiments. These are, however, of problematic value, for they never actually simulate natural conditions. Indeed, it is curious how uncritical many "experimental biologists" are in applying their work to conditions in nature.

The observations of Fridthjof Økland[6] shows the need of caution. Experiments of Beaudeant (1816) in which he gradually accustomed *Purpura lapillus* and *Patella vulgata* to entirely fresh water, suggested that these species might also become accustomed to fresh water in nature, making them valueless as a geologic index. Yet, in nature *Purpura* lived in summer only where the salinity was not lower than 20 to 25 parts per mille, whereas *Patella* evidently had a lower limit of 15 to 20 parts per mille. Økland's observations (as those of Beaudeant) are only for adult specimens and the halitaxis during the time of propagation as well as that of the larval stages should also be studied, since these are often at variance with the adults. It is a fault of work such as Beaudeant's that morphologic changes have not also been studied, although these may be of a kind and degree that are scarcely discernible.

The study of boreal Octocorals from the northern Pacific seems to suggest a possible way to elucidate these questions. In *Paragorgia* not the slightest difference seems to be present in specimens from the Atlantic and Pacific. In *Primnoa,* on the other hand, while the habitat is similar in places in the two oceans, it on the whole differs markedly. In this case, there is a tendency toward divergent variation in the two geographically separated groups. It is not possible to learn whether this variation is correlated with a change of ecological conditions (*i.e.,* whether the limit-

[3] "Zur Zoogeographie der Crustacea Malacostraca des Arktischen Gebietes," Zoogeographica, Bd. 2, Jena, 1935.

[4] "Ueber die amphiboreale (diskontinuierliche) Verbreitung der Meeresfauna in der nördlichen Hemisphäre," Zoogeographica, Bd. 2, Jena, 1934.

[5] Comp. Hans Krieg, "Kulturfolgende Tiere in Südamerika," Zoogeographica, Bd. 1, Jena, 1933.

[6] "Litoralstudien an der Skagerakküste Norwegens," Zoogeographica, Bd. 1, Jena, 1933.

ing environmental conditions are the same for the two oceans). It would also be interesting to learn whether *Paragorgia* lives under identical hydrographic conditions in the Atlantic and Pacific, since it is morphologically identical in the two habitats.

It is evident from recent investigations that the so-called boreal elements of the Atlantic are also represented in the northernmost Pacific by many species. Of course, endemic Pacific species will also be found, when the boreal region of the Pacific has been thoroughly investigated, but at present investigations especially along the American coast of the Pacific are insufficient. The limit of the boreal region in these waters is uncertain, though there is some evidence to suggest as a working hypothesis that its southern limit is near Puget Sound.

As far as we may judge from the scanty data at hand, the fauna of the Pacific waters along the northern part of the United States, the Canadian coast, Alaska and the Aleutian Islands exhibits a great similarity to that of the boreal Atlantic and boreo-Arctic regions, and this similarity opens up a large and valuable field of investigation for comparative zoology and ecology.

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DEATH OF THE OLDEST CHIMPANZEE IN CAPTIVITY

ON May 14, 1931, the Philadelphia Zoological Garden received a thirty-five-year-old male chimpanzee, "Jimmy," who claimed distinction of being the oldest of his species in captivity and of being the father of the first chimpanzee born in captivity (Anumá). Old "Jimmy" came with a reputation of being a "tough customer," up to which he lived for some time; then gradually he became quite manageable, and during the last months of his life he was gentle and friendly.

"Jimmy" for many years was a member of the primate colony of Mme. Rosalia Abreu, Havana, Cuba. After Mme. Abreu's death, in accordance with her will, he became the possession of the Philadelphia Zoological Garden, with the provision that upon his death his cadaver should be put at the disposal of Yale University.

Up to September, 1935, there are no notes on "Jimmy" at all; since then some records were kept, but these were inadequate because the present observer had under his care about a hundred animals with their diet, housing, mating and the like problems as well as a good deal of administrative work and was not able to give as much attention to "Jimmy" as he deserved. When observed in September "Jimmy" was not the animal known in the literature. He was not a morose, unmanageable old male, with ugly disposition and outbursts of violent activity, but a quiet, obedient, rather slow in movement senile chimpanzee with small and apparently weak muscles, wrinkled skin, poor pelage, sprinkled with white hair and bald patches. His appetite and digestion were remarkably good; he never failed to consume whatever food was given him, and his feces were always well formed. In October "Jimmy" occasionally left one or another article of food uneaten. Early in November his appetite failed more regularly and his digestion appeared to be faulty. His diet was modified, and in a few days "Jimmy" was as good as before. On November 15 the record says: "Very brisk this morning, swings and jumps." But soon he began to decline rapidly, his appetite, fairly good one day, was absolutely absent the next; he became inactive, would build a huge hay nest in the middle of the cage and rest on it most of the day. November 25 was the last day when "Jimmy" had a hearty meal; on the 26th he did not eat; on the 27th he vomited the food he had eaten on the 25th and, although the food was in his stomach for about 36 hours, almost every bit of it was easily identifiable at a glance. His feces during those days were soft but formed, and were whitish, due probably to increased intake of milk. On the 27th of all the foods given him, "Jimmy" took only half an orange, which he sucked dry, but did not attempt to chew. Milk was refused, but a good quantity of water was taken. Most of the day on November 27 "Jimmy" was lying in his hay nest on the floor in the middle of the cage. Only seldom did he get up and walk and then in the unsteady manner characteristic of the last few weeks. In the late afternoon he transferred some hay on the sleeping platform, carefully made up a nice, small round nest, and retired to it. At 7:00 A.M., on November 28, "Jimmy" was found dead on his big hay nest in the middle of the cage. He was lying on his left side; legs stretched and crossed; left arm flung out; right, almost parallel to the body; the right hand gripping some hay; eyes closed; teeth clinched; lips open, exposing the teeth.

Unfortunately our laboratory of pathology was denied the privilege even of cursory autopsy and therefore we are unable to say definitely what was the cause of "Jimmy's" death, except to surmise that it was "old age." The cadaver is now in possession of Dr. R. M. Yerkes, the director of the Psycho-Biological Department of Yale University, from whom we hope to learn the result of autopsy.

It is also unfortunate that the Psycho-Biological Department of Yale University, which carries on such an extensive program of research with chimpanzees, did not delegate from time to time one of its highly trained and experienced investigators to observe and

experiment with "Jimmy." The Philadelphia Zoological Garden, until a few months ago, neither had the facilities nor adequately trained observers nor, for that matter, any interest, to follow the physiological and behavioral changes that took place during the last years of "Jimmy's" life. And thus, from the viewpoint of a psycho-biologist, this very interesting and valuable subject passed away without enriching our knowledge. At present, under the leadership of the new director, Dr. R. Macdonald, steps are being taken to use the animals of the garden as something more than mere exhibits. It will, however, take some time to obtain and organize a sufficiently competent staff to carry out his plans.

"Jimmy" did not wait.

MICHAEL I. TOMILIN
PHILADELPHIA ZOOLOGICAL GARDEN

SELENIUM IN NATIVE RANGE PLANTS OCCURRING ON SOILS DERIVED FROM PERMIAN OR TRIASSIC (?) SEDIMENTS

SELENIFEROUS vegetation in Wyoming is, as has been pointed out before,[1] confined primarily to geological formations of Cretaceous and Eocene age. The Sundance formation, which is of Jurassic age, yields no seleniferous vegetation, at least from type sections examined so far.

During the past summer the Department of Research Chemistry of the University of Wyoming observed several hundred acres of seleniferous woody aster and narrow-leafed vetch growing upon red soils derived from the Chugwater formation in central Albany County, Wyoming. The Chugwater is in whole or in part of Permian or Triassic (?) age. Further inspection supplemented by chemical analyses brought out the fact that two horizons in the Chugwater formation were capable of supporting toxic seleniferous vegetation, one a sandstone member near the middle of the Chugwater formation and the other a limy sandstone in the basal portion of the Chugwater. The basal portion of the Chugwater in this locality is believed to be the stratigraphical equivalent of the "Embar" of central Wyoming. Of the two sandstone members it was found that the middle member carried considerably more selenium than the basal member. One sample from the middle sandstone member gave 10 parts per million of selenium. The average of a composite sample was found to be 2.4 parts per million.

The mere presence of selenium in rocks and soils of any geological formation has economic significance, in so far as native range plants are concerned, only when it becomes absorbed in quantities to be toxic. Finding seleniferous range plants in certain sandstones of Permian and Triassic (?) ages greatly enlarges the scope of the selenium problem.

O. A. BEATH
UNIVERSITY OF WYOMING

[1] O. A. Beath and co-authors, *Jour. Amer. Pharm. Assoc.*, 23: 2, February, 1934.

SOCIAL BEHAVIOR OF THE NORMAL AND CASTRATED LIZARD, ANOLIS CAROLINENSIS

EXPERIMENT reveals for the first time in any reptile species the presence of a true social hierarchy which differs in many respects from that of fowls. Nineteen males of *Anolis carolinensis,* ranging in weight from 7.5 grams to 3.9 grams, were found to arrange themselves in a series of dominance ranking which was closely correlated with weight. The largest males stood at the higher end of the scale (group I), the smallest at the lower end (group III), while those intermediate in size stood intermediate in the scale of dominance (group II).

Eight males in Group I ranked high in dominance with an average weight of 6.12 grams, five males in Group II ranked intermediate with an average weight of 4.96 grams, while the six males in Group III ranked low in dominance and their average weight was 4.73 grams.

Ninety-eight combats between males were carefully observed and recorded in detail. The males of Group I and II fought six encounters each, while those of Group III fought an average of three combats each.

The urge to acquire and to hold a certain restricted territory against other males was very marked. The resident male (one that had been in a particular cage for 24 hours or more) won 90 combats, but only eight fights were won by the non-resident (one introduced into the cage less than 30 minutes previous to the encounter).

This urge to hold territory was the more striking, since 38 encounters out of the total of 90 were won by resident males that weighed less than their opponents. The evidence indicates that resident males fight more viciously and more persistently than non-resident males.

The fighting pattern was found to consist of a chain of reflexes which, in a typical combat, at least, were expressed in almost an unvaried temporal sequence. Eight of these reflexes were expressed overtly, making it fairly easy to study the pattern objectively.

This fighting pattern has also been observed in castrated males and females but less in normal females. It is, therefore, inferred that the ovarian hormonal complex inhibits fighting reflexes in normal females.

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SCIENTIFIC BOOKS

QUANTUM MECHANICS

Introduction to Quantum Mechanics, with applications to chemistry. By L. PAULING and E. B. WILSON, JR., pp. xiii + 460. McGraw-Hill Book Company, 1935. $5.00.

DURING a decade of rapid progress in quantum physics, great advances in our understanding of a variety of physical phenomena have been made. Heisenberg's uncertainty principle and Bohr's principle of complementarity of the wave and particle concepts have called for as fundamental revision of basic concepts as was made twenty years earlier by the special theory of relativity. The details of the whole system of complex spectra of atoms have been fairly well worked out. Molecular spectra, both diatomic and polyatomic, have been interpreted and made to yield valuable information about chemical valence forces. The dielectric and magnetic susceptibilities of matter are now quite well understood in terms of fundamental atomic and molecular constants. The character of the coupling between electron spins that is responsible for ferromagnetism has been elucidated. In nuclear physics the main features of α-particle radioactivity were first made clear by quantum mechanics, and the methods are being carried over into other problems with success, especially where the heavy particles are concerned.

In the meantime the teaching of the subject to American graduate students has crystallized to a certain extent along the lines of the book under review. After a brief introduction to the methods of classical dynamics and a review of the pre-1925 development of quantum theory the student is introduced to the methods of quantum mechanics by way of Schrödinger's wave equation applied to simple mechanical systems. Chapter III explains the method in concrete terms by fully working out the harmonic oscillator, while Chapters IV and V give the extension to three-dimensional problems with a full presentation of the theory of the hydrogen atom. Chapter VI presents the perturbation theory for systems with discrete energy levels. Up to this point the selection of material and general treatment is like the older text-book of Condon and Morse, with, however, the advantage that much more detail of the mathematical work is included which should make it more readily understandable to beginning students who are not trained in the classical boundary value problems of mathematical physics.

From Chapter VII on the book begins to assume more specifically the quality indicated by the phrase "with applications to chemistry" in the title. Chapter VII is devoted to the variation method for approximating the solution of complicated problems. Chapter VIII introduces the non-relativistic treatment of electron spin and Chapter IX the theory of electronic structure of atoms containing many electrons. The rest of the book, except Chapter XV, the last, is devoted to a clear and elementary introduction to problems of molecular structure. The last chapter gives a brief account of the more general aspects of the theory than those that appear in the main parts of the work—matrices, the uncertainty principle and the general transformation theory. Confining itself strictly to what may be called the more elementary and "anschaulich" methods the book gives a clear and accurate account of a great deal of the important progress of the last decade.

As a general criticism I think that it is a disadvantage to present the general transformation theory as an isolated topic at the end. If introduced in an elementary way and documented properly with worked-out examples the picture of a state as a vector in function space which receives different explicit representations according to the coordinate system used is not too difficult for the beginning part of the work. And it is extremely illuminating to consider some of the formal processes of the theory from this standpoint. There is a great temptation to lean too heavily on "wave" methods in teaching the subject. That was the worst fault of the book of Condon and Morse, which is unfortunately not remedied here.

Any criticism which comments on the things that are left out must be taken in the spirit that the reviewer is crying for more. Of course the authors had to stop somewhere. Perhaps we can hope for a companion volume to this one by the same authors. Or if not by them then by others who will not waste energy redoing what is here well done but in going beyond to a fuller treatment of the things which had to be omitted to keep this book within bounds. Ortho-para hydrogen is not fully treated. The dipole moment and magnetic susceptibility of matter, recent advances in chemical kinetics, the statistical mechanics of molecular assemblies and the use of spectroscopic data for entropy calculations, the special features of vibrations of polyatomic molecules like vibrational resonance in CO_2 and the barrier-penetration coupling of vibrational levels in NH_3—these are a few of the topics which should be included in a companion work to this, now that students of physical chemistry have been provided with an introduction to quantum mechanics

which takes them as directly as possible to the field of their special chemical interests.

E. U. CONDON

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FUNGI

British Stem- and Leaf-Fungi (Coelomycetes). A Contribution to our Knowledge of the Fungi Imperfecti Belonging to the Sphaeropsidales and Melanconiales. Volume 1. Sphaeropsidales, to the end of the Sphaerioideae which have Colourless or Nearly Colourless Spores. By W. B. GROVE. xx + 488 pp. 31 text figures. Cambridge, England, at the University Press; New York, the Macmillan Company, 1935; $7.00.

AMERICAN mycologists who have used "British Rust Fungi," published by this author more than twenty years ago, will welcome the appearance of this new book. Though dealing with a different group of parasitic fungi, it resembles its predecessor in the view-point revealed, in method of presentation and in general appearance. Written to serve as a handbook in the British Isles, it will find a much wider field of usefulness, due to the extensive range of many of these organisms. It is to be followed by a companion volume which will cover the remainder of the Sphaeropsidales and Melanconiales. The Hyphomycetes will not be incorporated. The book is clearly printed on a good grade of paper and is attractively bound. It is inadequately illustrated, the few text-figures provided adding little to its usefulness. Good indices to host plants and to genera and species of fungi are given. Also there are included Latin diagnoses of twenty-four species described as new.

The author follows M. C. Cooke in the use of the old-fashioned and somewhat misleading name Coelomycetes. The older writer in his "Handbook of British Fungi," which appeared in 1871, discussed 200 species of these fungi. An indication of the tremendous increase in knowledge of the group is given by the statement of Grove that there are now 2,000 reputed British species. He calls attention to the economic significance of these fungi as "despoilers of our field crops, our orchards, and our woods," and emphasizes the fact that many fungi are actively parasitic only in their imperfect stage. The book is written, however, more from the standpoint of the mycologist than the plant pathologist. The descriptions of species stress morphological rather than pathological or cultural features.

Fifty genera are covered in this volume. Under each, the British species are arranged in definite sequence by host genera. Unfortunately, the host genera are listed alphabetically rather than systematically. This brings together the species occurring on species of a given host genus, but fails to place in proximity those to be found on related hosts. Each species is described briefly, and data covering host range, season of fruiting and distribution are incorporated. Relatively less space than usual is used in citation of exsiccati and other herbarium material examined. The author has been collecting these fungi for many years, and his personal collection of over 3,000 specimens has served as a basis for his work. In addition he has made the necessary comparisons with authentic materials in various historical herbaria. The book has the stamp, however, of having been written by a field mycologist rather than a herbarium worker. It fills a long-felt need for a handy reference work on these fungi.

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SPECIAL ARTICLES

BREEDING RUST-RESISTANT SPRING WHEATS

CALAMITOUS epidemics of stem rust occurred in 1904 and 1916 and again in 1935. Urediospores, overwintering in Texas, found optimum conditions for increase in their progress from south to north and finally the full force of the impact of the parasite fell upon the spring wheat fields of South Dakota, Minnesota and North Dakota. The loss in North Dakota alone, due mainly to rust, approached 100 million dollars. Before and following the epidemic of 1916 it was thought that catastrophes of this sort could be combatted by two methods, (1) the eradication of the common barberry, alternate host of *Puccinia graminis*, and (2) by breeding varieties of the other host, the wheat plant, which would be resistant to the parasite. After the 1935 epidemic, we know that breeding must be a major recourse.

Twenty years ago no variety of common wheat (*Triticum vulgare*) was known to have resistance to stem rust. The writers[1] discovered in 1917, in a durum introduction from Russia, plants of common wheat somewhat resistant to stem rust, which they selected and named Kota. Waldron crossed Kota wheat with Marquis, producing the Ceres variety, which has since become the principal hard spring wheat grown in the United States. Both the Kota parent and Ceres possess only moderate resistance to stem rust. While Ceres successfully withstood ordinary epidemics, it

[1] L. R. Waldron and J. A. Clark, *Jour. Amer. Soc. Agron.*, 11: 187, 1919.

suffered severely in the present epidemic but less than did its Marquis parent.

Greater resistance to stem rust is found in a few of the tetraploid wheats, the durums and emmers. Carleton[2] noted that some durum varieties and especially Yaroslav emmer (C. I. 1526) showed almost complete freedom from rust in the major epidemic of 1904, in contrast to marked susceptibility of others.

Starting with the rust-resistant durum Iumillo in 1914, Hayes, Parker and Kurtzweil,[3] in cooperative experiments at the Minnesota station, reported crosses with Marquis from which several rust-resistant hexaploid strains were obtained, one of which was increased and named Marquillo. While not possessing as much resistance as the Iumillo parent, the new variety indicated that successful rust-resistant common wheats could be obtained when a durum variety was used as one parent. As Marquillo carried the high carotin pigment of Iumillo in its flour the variety did not become a commercial success.

At the Dominion Rust Research Laboratory at Winnipeg, Manitoba, after the 1916 epidemic, the red-kerneled durum variety Pentad was crossed with Marquis. Some of the resulting selections carried the strong resistance of the Pentad parent, but because of lack of quality or yielding capacity none of the strains has been distributed.

In 1916, McFadden,[4] in South Dakota, crossed Marquis wheat and Yaroslav emmer (C. I. 1526) and by 1920 had isolated hexaploid strains, which later showed the same reaction possessed by the emmer parent in the mature plant stage. He distributed one of the strains, naming it Hope. While it possesses resistance to both rust and smut and has fairly good milling and baking qualities, it is not commercially successful, owing to susceptibility to heat and drought and to its lack of yielding capacity in ordinary years.

In addition to the cross which produced Marquillo, Marquis was crossed with the winter wheat Kanred,[5] and selections from a double cross Marquis-Iumillo × Kanred-Marquis were carried through in cooperative experiments at the Minnesota station. From among the many resulting strains one was named Thatcher and distributed by the Minnesota station in 1934. As an average of 4 trials in 4 localities in North Dakota in 1935, Thatcher averaged 23.2, Ceres 14.5 and Marquis 8.7, bushels per acre. Thatcher is the third named variety to be distributed having in its ancestry a resistant tetraploid wheat. Very recently the Canadian experiment station at Saskatoon has announced a new variety, Apex, derived in part from Iumillo and from H-44 (see below) through a complex cross.

Hope wheat has been crossed with susceptible and resistant common wheats, and a few of the immediate hybrid selections were promising in ordinary rust years in comparison with Ceres. The results from the heavy rust year of 1935 indicate that many of these hybrids, having Hope or H-44 (an allied strain of Hope) as one parent, carry essentially the near immunity of the emmer parent. The best measure of this in 1935 was at Langdon, N. Dak., where perhaps the epidemic reached its maximum intensity, and where Ceres and Marquis yielded only 3.3 and 0.4 bushels per acre, respectively, and the rust readings approached 100 per cent. Hope showed but a trace of rust, yielding 15.5 bushels, and various hybrids, with Hope as one parent, also had low rust readings and yielded as high as 18.7 bushels. Hope hybrids which have failed have done so mainly because of lack of drought resistance and yielding capacity, low bushel weight or susceptibility to certain diseases other than stem rust, such as black chaff. A few of these hybrids, such as Hope × Ceres, show good yielding capacity, as indicated at Langdon, and other desirable characters, especially quality. One of these immediate hybrid strains may be named and distributed for commercial growing.

Waldron has introduced the Australian variety Florence into a further cross, using with it Hope and Ceres. From this combination selections have been made which retain essentially the rust resistance of the Hope parent. In addition to this, the bushel weight is satisfactory and the yielding capacity good. In 1935 one of these Ceres × Hope-Florence strains yielded 23.3 bushels in the experiment at Langdon just mentioned. Apparently, in these hybrid selections, and doubtless also in others of different origin, it has been possible to retain the very important rust reaction found in the Hope parent and at the same time to eliminate those undesirable characters hitherto found associated with Hope or H-44 and with most of their hybrid descendants.

Besides Hope and Marquillo, only one named variety of excellent resistance, Thatcher, had been distributed previous to the 1935 epidemic, but in addition a considerable number of very promising hybrids were then under trial, and these met the crucial rust test with marked success. Thus this breeding program of 20 years' standing saw its culmination in the 1935 catastrophe. The emmer parent, which has supplied the near-immunity carried by most of the present hybrids, has been known for at least 50 years to possess this character, and there is every indication that it has behaved in this manner for a much longer period of

[2] M. A. Carleton, *U. S. D. A. Farm Bull.*, 219, 1905.
[3] H. K. Hayes, J. H. Parker and C. Kurtzweil, *Jour. Agric. Res.*, 19: 523, 1920.
[4] E. S. McFadden, *Jour. Amer. Soc. Agron.*, 22: 1020, 1930.
[5] O. S. Aamodt, *Phytopath.*, 17: 573–609, 1927.

time. One need scarcely fear that the reaction to stem rust now found in Hope and in so many of its descendants is but a temporary character.

When so many different physiologic forms of rust were being identified by Stakman and his associates it was held that breeding against rust had become a very complicated problem. At first it was felt necessary to breed for resistance to one form and later to groups of forms. Melchers and Parker[6] were the first to show that the reactions in wheat seedlings, used in determining rust forms, might not be the reactions of the maturing plant. Later this finding was amply verified for spring wheats by Goulden *et al.*[7] and by other workers. Thus, while Hope is susceptible to certain rust forms in the seedling stage in the greenhouse, it has shown freedom in the field from year to year both in the seedling stage and while approaching maturity. Generally the existence of many rust forms has not seriously complicated the breeding program.

The experiences of the past 20 years have taught many things that bear upon future breeding work. One of these is that farmers will not grow a wheat variety merely because it may be resistant to, or nearly immune from, stem rust. It also must have resistance to drought, must yield well and be of good quality; otherwise, the losses in non-rust years may overbalance the gains in heavy-rust years.

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EFFECT OF CYSTINE DISULFOXIDE ON SPONTANEOUS TUMORS OF THE MOUSE

THE principle that "The sulfhydryl—partially oxidized sulfhydryl groups comprise the chemical elements of a naturally occurring chemical equilibrium through which growth by increase in cell number is regulated"[1] having been established with vertebrates—and the stimulation side (–SH) in mice and men—a next step was trial of the retarding phase (–SO) in cancer growth of the mouse.

Preliminary tests with transplanted tumors had given results consistent with the postulate, but the data were inadequate in material methods and compounds.

Differential results with compounds not naturally a part of living economy are inconclusive—particularly so are results with sodium salts of inorganic sulfur acids, since these are unpredictably sensitive to oxidative and hydrolytic changes in water solution. Extension of the principle to vertebrates depends on demonstration of proliferation retardation by partially oxidized derivatives of a *naturally occurring* sulfhydryl compound.

Preparation of these is a research problem in itself. Three years ago report was here made of the retarding action of one such—a cystine sulfinate—on proliferative growth in *Obelia gen.*[2] This was not available in sufficient amount or purity for other trials. Since that time Drs. Toennies and Lavine have prepared a cystine disulfoxide in pure form and adequate amount. They will report the chemical details elsewhere.

This note is concerned with the growth of 91 spontaneous tumors from mice injected almost daily with 0.0085 gm. of cystine disulfoxide, as compared with that of 65 tumors from untreated mice simultaneously living under like conditions of diet and environment.

The test data show that the administration of this amount was accompanied in this experiment by a lesser maximum tumor size, a lesser maximum percentage increase in size, a slower tumor growth, fewer tumors reaching a stated percentage increment, tumors with fewer cells, tumor cells with larger nuclei, a greater red cell anemia at the end and a 25 per cent. prolongation of life beyond that expected from the control data. There was no essential difference in initial maximum or terminal body weight between tests and controls.

Of significance is the fact that cell nuclei in tumors from injected mice were larger than those of controls. This result is identical with that found in other material and is correlative evidence of proliferation retardation.[3] The fact—which always appears in work of this sort—that cell size is smaller when proliferation is speeded up by sulfhydryl and larger when retarded by its partially oxidized derivatives is evidence incontrovertibly inconsistent with any hypothesis which would attribute to sulfhydryl a forwarding action on assimilatory protein synthesis—or growth by increase in cell size or mass.

Since these results demonstrate the proliferation retarding action of cystine disulfoxide in mice, they allow extension to vertebrates of the postulate developed from earlier work.

The degree of change from the usual, however, though consistent on all counts, was too small to allow any prediction as to future practical possibilities.

This was a cooperative work. Conduct of the mouse experiments was guided by S. P. Reimann. Solutions

[6] L. M. Melchers, J. H. Parker, *U. S. D. A. Bul.*, 1046, 1922.
[7] C. H. Goulden, Margaret Newton and M. A. Brown, *Sci. Agric.*, 11: 9, 1930.
[1] *Protoplasma*, 11: 382, 1930.
[2] SCIENCE, 77: 190, 1933.
[3] *Protoplasma*, 7: 535, 1929.

were made by T. Lavine. To Miss Hall belonged the care, injecting and measuring of the mice. Sections were made by Misses Chatalbash and Kiesel. Analysis of the results was the task of F. S. Hammett. Merck and Company generously supplied needed chemicals, and much support was given by The International Cancer Foundation.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

AN EASILY CONSTRUCTED RELAY

BLACK[1] has described a relay which possesses the features of being both inexpensive and efficient when using power up to 1.5 KW. A worker in this university has constructed a relay, employing the same principle Black used in his relay but has modified the construction.

When the current was broken by the thermoregulator in Black's relay, a plunger dropped by gravity on the column of mercury in one side of the glass tube and pushed the mercury up on the other side so that it made contact with a platinum lead. To construct the glass tube described by Black, considerable knowledge of glass blowing must be at the command of the technician.

The relay described below is much simpler to construct and any person with an elementary knowledge of glass blowing can build one. We have incorporated an entirely different principle in our relay in that the plunger *C* (see diagram), when dropped displaces mercury instead of pushing it up the other leg of the glass tube. By displacing mercury, the level is raised in a straight piece of pyrex tubing and electrical contact is made. We have retained in our relay, however, Black's magnetic coil method of raising the displacing plunger.

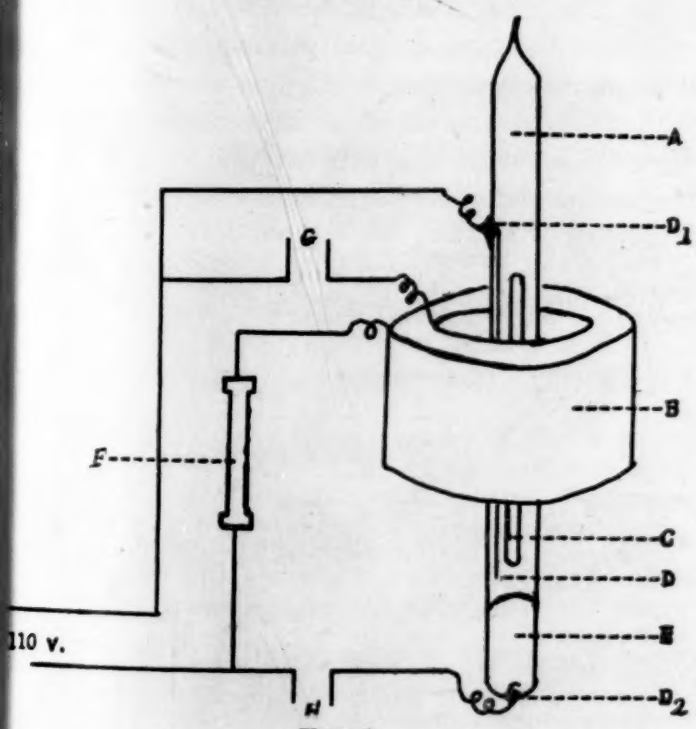

FIG. 1

A —Pyrex tube
B —Secondary of Ford coil
C —2 mm × 4 cm iron nail
D —Platinum wire, 22 ga.
D_1—Platinum contact
D_2—Platinum contact
E —Mercury
F —1000 ohm resistor
G —Leads to regulator
H —Leads to heating unit

The pyrex tube *A* (see diagram) has a 7 mm bore, and is 10–12 cm long. At D_1 and D_2, 22 gauge platinum wire is sealed directly into the glass. Wire *D* is long enough to reach within 7–8 mm of the mercury *E* when the plunger *C* is raised. The plunger is a number 6 finishing nail and is inserted with the head down. When the plunger is lowered into the mercury, the level of the mercury is raised and contact with wire *D* is made. *B* is a secondary coil from a Ford model T induction coil. When operated from a 110 v. A.C. or D.C. line with a 1,000 ohm resistor *F*, there are 35–40 milliamperes of current flowing through the thermoregulator. If resistance of 1,500 ohms is used the current can be reduced to 30 milliamperes. (A resistor of the type used in radio work is satisfactory.) This is sufficient current to operate the magnetic coil.

When contact is made in the thermoregulator at *G*, current flows through the coil *B* and creates a magnetic field which lifts the plunger out of the mercury, and contact between the point *D* and the mercury is broken. Conversely, when the contact is broken in the thermoregulator, the magnetic field disappears and the plunger drops by gravity into the mercury, and contact between point *D* and the mercury is made, thereby completing the heater circuit. It is important that the wire *D* does not touch the side wall of the glass tube as the lower portion of the tube becomes plated with mercury and contact between *D* and *E* is made through the plating. Instead of placing points in an atmosphere of hydrogen as described by Black to prevent oxidation, we obtained, with 550 watts, very satisfactory results by merely evacuating the tube before sealing. By protecting contact points D_1 and D_2 with a condenser of suitable capacity for the current required, sparking is eliminated.

This relay was used daily for a period of 4 months at the end of which time no noticeable signs of deterioration were observed. This can be adapted for use

[1] Peter T. Black, SCIENCE, 79: 322, 1934.

wherever a sensitive mercury relay is required. The total cost of this relay is less than two dollars.

We wish to thank Dr. B. R. Stephenson of the Buffalo City Hospital for his valuable suggestions and comments.

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HOLLOW-GROUND SLIDES FOR WHOLE MOUNTS MADE WITH THE DENTAL ENGINE

THE customary method of making slide mounts of whole specimens requires supporting the cover glass to prevent crushing the specimen, and results at best in a thick preparation which hardens slowly and is easily damaged. Commercial hollow-ground slides are satisfactory in some cases, but their concavities are too large for many specimens and their costliness puts them out of the question for routine use. A simple solution of the difficulty is afforded by the use of a motor-driven flexible shaft and grinding tools, such as the well-known dental engine. Slides with depressions to fit any specimen may be easily and rapidly made as required. These mounts permit the cover glass to rest directly upon the surface of the slide, require a minimum of mounting medium and facilitate the orientation of the specimen. Where a number of specimens are to be mounted under one cover glass, as for example the successive stages of an insect, their arrangement in any desired position is made possible by grinding a depression for each specimen.

The essential apparatus for turning out these laboratory-made slides is an electric motor (that of an electric fan will serve), a flexible shaft provided with a chuck or "handpiece" into which may be fitted any of the dentist's arsenal of burrs, drills and abrasive devices. Of these the most generally satisfactory for grinding glass are the abrasive wheels, which consist of small disks of carborundum or other material mounted on a mandrel, and which are available in a variety of diameters, thicknesses and degrees of abrasiveness. Abrasive "points," *i.e.*, small carborundum spheres, cones and cylinders, may also be used, but are much less rapid than the abrasive wheels on account of their small diameter and hence low velocity of grinding surface.

The process of grinding a depression consists merely of placing a drop of water on the slide and applying the abrasive instrument. Very little spattering occurs. The most rapidly ground depression is the slot made by the edge of the carborundum wheel. A cavity of this shape is desirable for elongate specimens. By moving the wheel while grinding, a depression of almost any size and shape may be made, and rotating the slide on a turn-table produces a circular concavity similar to that of the ordinary hollow-ground slide. Where any considerable quantity of glass is to be removed the wheel is the most efficient instrument, though the small carborundum spheres which grind circular depressions with relatively steep sides give the neatest and most uniform results. An inspection of the grinding devices at a dental supply house or in a dentist's office will doubtless suggest at once the instrument appropriate for a given purpose.

These slides may be ground with a speed which permits their routine use for large series of class slides. For example, a slot-like depression suitable for a mature flea larva may be ground in ten to fifteen seconds, one for a mosquito larva in about thirty seconds, while the broader and deeper depression necessary to accommodate a bedbug may be ground in about a minute. The ground surface may be polished in a little more than the original grinding time, though polishing is usually unnecessary since the ground surface is nearly invisible when covered with the mounting medium.

Once a flexible shaft and its accessories are installed in the laboratory it will be found to serve any number of useful purposes besides grinding slides. Among these may be mentioned etching glass or metal, making the opening in egg-shells for the purpose of inoculating chick embryos, and grinding and shaping dissecting needles and other instruments under the dissecting binocular.

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